DOCUMENT RESUME

ED 058 106 SO 001 514

AUTHOR Milburn, Thomas
TITLE Problems of Integrating Academic Disciplines in the Study of War, Violence, and Social Change.
INSTITUTION Diablo Valley Education Project, Berkeley, Calif.; New York Friends Group, Inc., New York. Center for War/Peace Studies.
PUB DATE 70
NOTE 19p.; Paper presented at a conference, the Utilization of Scholarship in Teaching about War, Peace and Social Change, San Francisco, California, March 21-22, 1970

EDRS PRICE MF-$0.65 HC-$3.29
DESCRIPTORS Aggression; Antisocial Behavior; *Behavioral Sciences; *Conflict; Cooperative Programs; Curriculum Development; Human Relations; Intellectual Disciplines; *Interdisciplinary Approach; Research Needs; *Social Change; Speeches; *Violence
IDENTIFIERS Peace; War

ABSTRACT

The problems mankind faces are of such overwhelming importance that it is easy to see why we are interested in integrating the academic disciplines to study war, violence, and social change. Could not the behavioral sciences, properly mobilized, enable us to reduce the probabilities of war and violence, and make social change more tolerable? Toward these ends, the integration of academic disciplines, and the cooperation of these disciplines with practitioners (educators) should prove worthwhile. There are several obstacles to interdisciplinary work: 1) the anxiety-provoking nature of this area of research, which causes people to withdraw to safe problems in their own disciplines; 2) the tendency to regard only the things in one's own discipline as problematic, and those things outside as givens; 3) the defensiveness and jealousy that often exists between disciplines; and, 4) each discipline having its own distinct methods. One way to overcome these obstacles is to be aware of the conceptual convergences that exist across disciplines. These convergences set the stage for cooperation. There are new methods and concepts that make cooperation easier, e.g., the man-computer simulation to study conflict. It is not obvious how such projects can best be organized, and we need to collect critical incidents concerning successful attempts at cooperation. (Author/JLB)

ED058106

SP001514

"PERMISSION TO REPRODUCE THIS COPYRIGHTED MATERIAL HAS BEEN GRANTED BY

Robert E. Freeman

TO ERIC AND ORGANIZATIONS OPERATING UNDER AGREEMENTS WITH THE U.S. OFFICE OF EDUCATION. FURTHER REPRODUCTION OUTSIDE THE ERIC SYSTEM REQUIRES PERMISSION OF THE COPYRIGHT OWNER"

PROBLEMS OF INTEGRATING ACADEMIC DISCIPLINES IN THE STUDY OF WAR, VIOLENCE, AND SOCIAL CHANGE

THOMAS MILBURN

Department of Psychology
DePaul University
Chicago, Illinois

U.S. DEPARTMENT OF HEALTH, EDUCATION & WELFARE
OFFICE OF EDUCATION
THIS DOCUMENT HAS BEEN REPRODUCED EXACTLY AS RECEIVED FROM THE PERSON OR ORGANIZATION ORIGINATING IT. POINTS OF VIEW OR OPINIONS STATED DO NOT NECESSARILY REPRESENT OFFICIAL OFFICE OF EDUCATION POSITION OR POLICY.

The materials herein are part of the work of the Diablo Valley Education Project, in the Mt. Diablo Unified School District, Contra Costa County, California. Requests for permission to reprint this material or requests for additional information on the work of the Project should be sent to:

Diablo Valley Education Project
Center for War/Peace Studies
50 Vashell Way, Suite 300
Orinda, California 94563

or

Diablo Valley Education Project
Center for War/Peace Studies
218 East 18 Street
New York, New York 10003

© 1970 Center for War/Peace Studies of the New York Friends Group, Inc.

PROBLEMS OF INTEGRATING ACADEMIC DISCIPLINES IN THE STUDY OF WAR, VIOLENCE, AND SOCIAL CHANGE

Thomas Milburn
Department of Psychology
DePaul University
Chicago, Illinois

It is not too difficult to see why we might be interested in integrating the academic disciplines in the study of such phenomena as war, violence, and social change. The topic is unbelievably pertinent to problems which confront and imperil civilization and humanity. These categories are not mutually esclusive; e.g., wars include violence although personal and small group violence do not imply war. Social change contributes to the frequency of war and non-war violence. Wars and violence may produce social change. Consequent changes in societal values may reduce inhibitions toward violence within or without the societies in which they occur.

Wars grow more expensive, especially major ones. The economic costs of World War II were an order of magnitude greater than those of World War I (Sprout, 1963). In a lucid, cogently reasoned essay, Harrison Brown (1966) has argued that because of ecological skimming of our planet's natural resources, the birth and rise of technological civilization after a World War III could not reoccur. It is conceivable his argument is optimistic: maybe not only technological civilization but also mankind would be too changed to rise again after another major war.

One small, undeclared war in southeast Asia costs well over thirty billion dollars a year, has taken fifty thousand American lives alone, and may be producing profound value changes at home. It is a principal cause of inflation, and by the ecological and capital drain it has put upon U. S. resources, it has contributed to urban blight and to reduced rates of the accumulation of capital and reduced funding of education and research. Some argue that it makes us accept violence more readily.

Violence within countries now appears as one indicator of the prevalence of the social system's pathology, akin therefore to readings of the pollution levels of air or water, of levels of radiation, of levels of hunger, or of the frequency of various disease syndromes. The Feierabends (1966) have discovered relations between reliable estimates of need to achieve within a society and increased frequencies of violence when achievement needs do not lead to increased rates of modernization. Increased rates of violence within societies may reflect the dissolution of emotional bonds between men and the social institutions - family, church, lodge - of society together with the bonds to formerly valued norms - rules, laws, and moral values. Predatory lethal violence to strangers, followed by neither strong remorse nor suicide, is greater in the U. S. than in all other developed countries of the world. We also excel substantially for deaths by violence to family members and friends (West, 1966).

The problems represented by the phenomena under discussion are thus substantial ones. We realize that physical science has been remarkably successful in producing vast technological change. Could not the social and behavioral sciences, properly mobilized, achieve equivalent wonders in enabling us to reduce the probabilities of war and

widespread violence? And make social change more tolerable? If the physical sciences can harness natural forces, cannot the social sciences help us control dangerous social pathology?

Toward those ends, the integration of academic disciplines might prove most worthshile; and maybe the time for more interdisciplinary cooperation is not far away. There already appear to be conceptual convergences across the disciplines: e.g., although the hypothesis that frustration produces aggression has recently suffered some truncation by psychologists, it also has been utilized by political scientists with some success (Feierabends, 1966). And there are many others: the generality of social scientific hypotheses and concepts advances.

However, it is from the differences among the academic disciplines, through their diversity and heterogeneity that mankind may produce the most gain. Each discipline represents a separate pattern and range of human resources - separate diversities of skills, knowledge, talents, and methods. It is true, moreover, that complex, many faceted problems yield more readily to a variety of abilities than to a single kind. Some kinds of problems are solved more readily by heterogeneous groups than by individuals.

The utilization of several research strategies in concert, e.g., surveys, content analysis, field work, the clinic, computer simulation, and laboratory experimentation, promises to reduce the error or unreliability of measurement associated with any particular method. Technically put, the use of several methods to test a set of hypotheses reduces <u>method error variance</u> (Campbell & Fiske, 1959). What new knowledge emerges is therefore likely to prove both more certain and more general than what might have been produced by a single discipline.

Aside from the fact that convergent findings indicate a stronger, because diverse, evidential base, several methods together offer a strong basis for convincing policy makers of the truth of empirical findings. Policy makers and other practitioners tend to have been educated in only one of several disciplines and to respect some forms of inquiry and evidence far more than others. The physical scientist respects mathematics and systems approaches; historians appreciate historical data and analogs. In general, each man respects forms of inquiry and evidence which he best understands and with which he is most familiar. Multiple research strategies are likely to yield evidence to satisfy men with very different training and backgrounds.

What I have said thus far may imply a belief that the academic disciplines could "go it alone," could, on their own, contribute materially not only to the understanding of more and less legitimated forms of violence and social change, but also to their resolution or successful management. I do not hold any such belief. Inevitably practitioners must act to utilize whatever new findings are generated, or those findings do not constitute effective knowledge possessing social relevance. (Any new knowledge concerning violence and social change has the quality of social relevance.) What is a prime essential is that members of various academic disciplines concerned with these matters meet together with practitioners, as we are doing now, to review what we feel we know, but also to question premises, look for interrelations, to decide anew what appears most problematic, and what queries most deserve attention. Practitioners or policy makers can serve as catalysts to increase the probability that academicians will work well together, especially if they help jointly to pose queries relevant to making

decisions and taking actions. Moreover, research which involves some practitioners from the initial stage of formulating questions stands a better chance of being utilized sooner than later. And at the risk of sounding a prophet of doom, I must say that it looks very clear that mankind's days may be numbered unless we can solve man's over-determined penchant for self-destruction.

If cooperation is so desirable, what then are the major obstacles to its attainment; and how shall we deal with them? First, there is a general one related to the phenomena which interest us: like research on death, suicide, racial prejudice, religion, or sex, war and violence are subjects hard to be dispassionate and objective about (Farberow, 1963). They arouse anxiety in many researchers who, gazing at change and violence, fear both their own feelings and the untractability of the subject matter - as contrasted with their reactions to inquiries into, say, resource allocation, problem solving, or verbal learning.

What is more important is the fact that the various academic disciplines are often jealous of and defensive toward one another. Scholarly territoriality can look very real as psychologists or sociologists protect their domains. The existence of clear status differences across the disciplines unfortunately does not help. Members of one discipline can resent the very possibility that they may "consult" for members of another or that their findings will appear in appendices. The most interesting tasks with the highest status involve formulating the questions.

Each discipline tends to have its own distinct methods of study, tailored to fit its self-defined objects of inquiry. Methods are normative, however, and like other normative objects, can lead to some very

strong emotional attachments. Psychologists mostly believe that evidence from the experimental laboratory is the very best and may disparage the existence of phenomena which are not readily amenable to laboratory investigation. The clinical concept of repression, i.e., anxiety-escape motivated forgetting, comes readily to mind. Sociologists cherish and admire field observation as the first method. They prize understanding processes. Political scientists manipulate archival documents or utilize content analysis. Members of one discipline do not often study nor readily appreciate the methods of another. This "fact" too hinders mutual respect and cooperation.

Each discipline tends to regard the phenomena it studies - at the level of analysis it employs - as problematic and all else as given. So each discipline looks at endogenous factors and ignores exogenous ones. Economists consider economic factors as endogenous and problematic and non-economic ones as exogenous ones, givens which are not to be investigated. Psychologists look at intra-organismic or (at largest) at interpersonal phenomena as problematic and worth investigating: economic, cultural, political, or historical factors are for them exogenous givens, outside the purview of their disciplinary charter. It is always easy to forget that the nature of each discipline has an arbitrary quality about it, that it exists in the shape it does, with the subject matter upon which it concentrates, with its distinctive premises - as the result of certain historical accidents, i.e., the interests of its founders and developers.

Most of the above factors which I suggest are obstacles to interdisciplinary research because they make mutual respect more difficult and mutual defensiveness easier are sociological ones. They are also

more purely philosophical ones. As economist Mancur Olson has put it:

> The work of economists in international studies often involves the assumption that a nation's behavior is capable of being described in terms of the rational pursuit of determining interests. The object of the analysis is typically to determine the most effective or efficient methods of attaining goals. By contrast, the sociological mode of analysis tends to concentrate on the influence of socialization on the behavior of nations and political leaders, and the problem of determining a rational strategy for attaining determinant goals tends to receive little attention. Where the causes of peace or war are at issue, the economist tends to think primarily of the mutual advantages attained from avoiding war (e.g., the theories of deterrence and balance of power and from international trade). The sociologist, by contrast, often tends to emphasize the fact that common ideologies or religions reach across many national borders, the negotiating styles of diplomats, and international laws and normative precepts.

While presumably equally scientific, the disciplines may differ remarkably in terms of their premises and approaches. At least at present a psychologist interested in the experimental analysis of human behavior would refuse to have any traffic with non-observalbes as are reflected in the (also non-minipulable) cognitive categories of the sociolcgist such as perceived or understood consensus. A term like legitimate or legitimacy is rarely found in the indexes of psychology texts.

The obstacles I have described are formidable ones. Not only are members of different disciplines suspicious of strangers to them, protective of their own domains, and occasionally defensive about their possible weaknesses. They also so differ with respect to methods, premises, concepts (and the philosophical roots of their concepts) that they have difficulty in understanding one another quite as much as they have trouble trusting one another. Their differences in ways of looking at problems, in the units they employ for observation and analysis, and so on, are their strengths, cumulatively built to enable them to deal

with their favorite dependent variables. These very strengths are also obstacles in the way of interdisciplinary cooperation even for those disciplines which concentrate their energies upon the study of man. Their obstacle quality is reinforced by the organization of universities into academic departments which themselves structure academic careers. It is also reinforced by the various professional societies which strive to emphasize separate and unique professional and scholarly identities.

There are ways for us to overcome obstacles to interdisciplinary integration vis-a-vis conflict. Translations of similar meaning terms across disciplines do seem feasible though terminological meanings only partially overlap. There is, for example, the non-zero sum game which can result in win-win or lose-lose relationships in which both parties to a relation come out ahead or behind depending upon whether the cooperative or the conflictual elements of a relation come to be most emphasized. Win-win games look analogous to the principle of comparative advantage in international trade, to the concept of role complementarity in sociology, the creative relationship in psychology, to constructive symbiosis in biology. Lose-lose games look like neurotic or psychotic relationships in psychology, to role conflict in sociology, to mutual parasitism in biology. It seems plausible that explorations of the similarities and differences in the meanings of these terms for example could suggest worthwhile research to contribute to our understanding of conflict. Certainly the exercise could contribute to communication among the disciplines, assuming that men across disciplines interested in conflict would be motivated to attempt the initial exercise. These terminological difficulties do not look impossible to overcome, and confronting them would seem potentially very fruitful.

Except for man-computer simulations, phenomena like war, have not been encapsulated in the laboratory. They are emergent phenomena which appear at only certain levels of societal complexity. Still, phenomena which lead to wars, e.g., crises, may be studied at various levels of social complexity, with many units of analysis and through the utilization of a wide variety of methods. A better understanding of the distortions of perception, decision making, and action, which occur in real or artificial crises can be expected to benefit the advance of knowledge across several disciplines and perhaps be of worth to practitioners through reducing vulnerability to crises. Note, however, that mutual respect and some ability to communicate across disciplines is a prerequisite for easy collaboration at empirical or conceptual levels on related problems, especially when we cannot tell where major gains will appear - if, indeed, they show at all.

It is still not altogether obvious how projects on the investigation of phenomena such as those we concern ourselves with here can best be organized. We need to collect critical incidents concerning notably successful or unsuccessful attempts at cooperation. I imply that we can gather evidence of the success or failure of collaboration. How shall we measure success - no more wars, less violence? Those are all right, but others are necessary too; I suggest several possible indicators of success or failure of efforts.

(1) Scientific: is new theory developed - or concepts, methods; is new research inspired as a result?

(2) Pragmatic: are new problems solved or old ones solved more readily?

(3) Public Policy: what evidence exists that these efforts can make or have made a difference on the actions or decisions of policy makers? Have such impacts been at all institutionalized (have they resulted in organizational learning), or must other projects start from scratch?

The obstacles to interdisciplinary cooperation and to men of action and men of knowledge cooperation are formidable ones, psychological, sociological, and conceptual. Except for some of the inhibitions I have suggested above, it is surprising that there have not been more studies of change, conflict and cooperation among the disciplines. Certainly we have much to learn, and such investigations might suggest hypotheses toward the general understanding of processes of conflict together with means of resolution. The problems we and the world face are of such overwhelming importance that we must not regard the obstacles of interdisciplinary and practitioner-academician cooperation as insurmountable. If we succeed, we all gain. If we fail to work together, mankind may go down the drain.

REFERENCES

1. Brown, H. The Challenge of Man's Future. New York: Viking Press, Compass Books, 1966.

2. Campbell, D. T. and Fiske, D. W. Convergent and discriminant validation by the multitrait-multimethod matrix. Psychological Bulletin, 1959, 56, 81-105.

3. Farberow, N. L. Taboo Topics. New York: Atherton Press, 1963.

4. Feierabend, I. K. & Feierabend, R. L. Systematic conditions of political aggression: an application of frustration-aggression theory. Paper presented at the meeting of the American Association for the Advancement of Science, December, 1966.

5. Sprout, H., & Sprout, M. Foundations of international politics. Princeton, New Jersey: Van Nostrand Co., 1963.

6. West, D. J. Murder followed by suicide. Cambridge, Massachusetts: Harvard University Press, 1966.

THOMAS MILBURN: "PROBLEMS OF INTEGRATING ACADEMIC DISCIPLINES IN THE STUDY OF WAR, VIOLENCE AND SOCIAL CHANGE"*

The problems we face here are quite big - so big they really are overwhelming. Moreover, the problems are multi-faceted, and I don't see the possibility within the intellectual capabilities of any one discipline to attempt to solve them. One of the interesting things about war, violence and social change is that they all still contain so many aspects that are problematic - that is, whatever we may say that we know about them, there is still a lot that we don't know. And this, of course, is one of the reasons for us to be concerned with working together. The problems may be immense, but it is this multi-faceted nature of the problems that seems to me really calls for interdisciplinary research. Problems that are complex often yield more readily to heterogeneous groups; if you have a group of people, all of whom are bright but with overlapping knowledge and diverse skills, you do much better at solving problems than otherwise. It takes more than diversity that way. It also takes cooperation, which is hard to generate.

None of this is to imply that the disciplines should merely work together even in an interdisciplinary approach. The disciplines should have to cooperate with practitioners as well. In this case it seems to me the practitioners clearly are educators and the educators, among others, very much need to be invited in and involved in formulating the research problems.

One obstacle to interdisciplinary work is that the particular area of research we are concerned with produces anxiety in many people, and with anxiety they retreat to the security of their own fields. Thus conflict and violence are like other areas such as religion or sex, each of which is also hard to study because it arouses anxiety resulting in withdrawal to safe problems in one's own discipline. Note, by the way, that in order to cooperate it is necessary to be daring, to take a chance, to respect others even when you see that when they talk about your area they do not know as much as you do. You have to find some criteria by which you can decide to trust them when they are talking in areas about which you don't know so much and about which it can be harder to judge them. Each research area has its own methods and some of the strongest emotional commitments I know are to methods.

Another problem, it seems, is the tendency to regard the things that we work on in our own discipline as problematic: i.e., 'these are the problems and everything else is a given.' For example, the economist

---

*Thomas Milburn, department of Psychology, DePaul University. *This paper was presented at the Inquiry, "The Utilization of Scholarship in Teaching about War, Peace and Social Change," March 1970, San Francisco, sponsored by the Center for War/Peace Studies, in cooperation with the American Orthopsychiatric Association, the International Studies Association and the Diablo Valley Education Project.*

who studies developing countries usually regards sociological factors as exogenous; he doesn't even look at them.

One way to overcome these obstacles is to be aware of the fact that there are conceptual convergences that exist across disciplines. One in particular seems quite relevant to conflict, the notion of the non-zero-sum game which can result in win-win or lose-lose relationships in which both parties come out ahead or behind. Win-win games look analogous to the principle of comparative advantage in international trade, to that of role complementary in sociology, creative relations in psychology, constructive symbiosis in biology. And lose-lose games look like neurotic or psychotic relations in psychology, role conflict in sociology or a kind of mutual parasitism in biology; they may also look like wars.

These conceptual convergences exist independently of cooperation. But they set the stage for cooperation across disciplines, and they suggest that we really do have something to communicate about if we can get into it, if we are willing to tolerate the fact that we have somewhat different languages. It takes a while to be aware that we are closer than we knew. We can abet cooperation if we concentrate first on similarities across disciplines (for example, the concepts) before emphasizing differences.

There remain many different kinds of things to be done, and there are new methods and new concepts that make cooperation easier. For example, the man-computer simulation can prove a good way to study conflict. Another is this whole notion of system - putting together hypotheses that have some meaning at different levels of societal complexity.

These are ways to talk and work together. We still, then, are faced with the problem of sitting down and realizing it is going to be tough and it is going to be abrasive and we are going to have a hard time tolerating one another. But this game is worth the candle.

DAVID HAYS: COMMENTS ON THOMAS MILBURN'S PRESENTATION,
"PROBLEMS OF INTEGRATING ACADEMIC DISCIPLINES
IN THE STUDY OF WAR, VIOLENCE AND SOCIAL CHANGE"*

I don't know how many of you have had the experience of inserting a hand between a knife and its target, but that happened to me a couple of weeks ago. The young man who was wielding the knife, a straight bladed knife, a knife at least six inches long, was trying to use it to pick the lock on the door to the President's office at my University, and I had to put my hand in between even though he said then that he does believe in the use of violence. But it turned out that he didn't believe in it enough to do it when the case came. I'm going to take a minute to tell you how I got into that situation and then say a few things about what I think this interdisciplinary research might bring to a related problem.

I started getting into that situation by going to Harvard when I was 19 years old; I got a Ph.D. in Sociology there. Since then I have been with the Center for Advanced Study; with the RAND Corporation working on studies of radar networks and then, for ten or twelve years on computational linguistics; and about a year-and-a-half ago I went to New York University at Buffalo to organize a Linguistics Department. I had, in the meantime, invented the ultra-microfilm library and decided I was neither a scholar nor a researcher but an inventor by trade. I don't belong to a discipline anymore; I had belonged to the psychological, sociological and statistical associations for a long time; I've done mathematical modeling and computer work; and I've worked in the field of education for Encyclopedia Britannica. So I find now that I am really a generalist.

I do not believe that you can train interdisciplinary specialists from the beginning. I think that everybody's got to have one, two - some disciplines and then he can say he is a generalist. I also feel that if I hadn't bought my new position - my new position is that I'm not going to do any work any longer - and if I hadn't paid for that with twelve or so years of hard detailed effort, then I think maybe I wouldn't deserve it.

I came to the University, then, for the first time when I was about forty years old to take my first full-time university job. And I took it with the certainty that every professor has a paternal obligation to his students. So that in the first year I stood between a column of police and a line of strikers, and when the strike came up this year I knew I had to get into it somehow. And inevitably I'm involved. I feel

David Hays, Department of Linguistics, New York University at Buffalo. These comments were made at the Inquiry, "The Utilization of Scholarship in Teaching about War, Peace and Social Change," March 1970, San Francisco, sponsored by the Center for War/Peace Studies, in cooperation with the American Orthopsychiatric Association, the International Studies Association and the Diablo Valley Education Project.

that we are all inevitably involved in the things around us. And to walk away and close your eyes and say you won't worry about it is just impossible.

Now the most important thing to remember about interdisciplinary work, it seems to me, is that whenever you tackle a problem and say you will put an interdisciplinary effort into it, you are by that act creating a new discipline which will have its view of what reality is - which will have its theory that draws on the theories of all the disciplines put together. Its specialty will be the examination of interactions among variables that are the properties of the several disciplines which you draw on to create it. None of the disciplines is interested in the interactions of variables - some inside and some outside their fields - and so the new field has this specialty of its own, these interactions that nobody previously had thought about working on.

For example, as you think about applying solid science or basic knowledge to practical problems it seems to me that you have to forgive the solid scientists for disclaiming the responsibility for making the transfer to the practitioners themselves. The scientists are not specialists in the transfer of knowledge from their level of abstraction to that of the practitioner. There is a new specialty: the specialty of the people who write review articles. Some articles are by scientists for scientists, but some are by specialists in gathering up knowledge and rewriting it in forms that make sense to practitioners. Now, that's not an art that a lot of people know, but it is one which could prove invaluable to our work here.

Finally, I would like to propose a kind of experimentation which it seems to me the new discipline of conflict studies might induce. There are beautiful examples of violent conflict and semi-violent conflict on our campuses. The story I began with here is a perfect example of this. And the thing is that the protagonists on both sides are us and our friends. And therefore, it's possible to be in touch with the leaders in the administration and the faculty, and among the striking, rioting students. Now, the anti-rationalist bias of the students, particularly the ones who are likely to be on strike or building riots, is fairly strong. But I think it may be possible to get through to them and convince them to study themselves as they carry on the riots so that society can understand them and they can understand themselves better. And they can then see more clearly what their goals really are and what mechanisms for reaching their goals are most efficacious.

So, to summarize, we need to recognize that interdisciplinary work, if it is to be successful, creates a new discipline out of the two or four or however many inputs there are. And one purpose for one such new discipline could be the study of conflict, with an eye to non-violent solutions of situations like the one I was involved in as opposed to choosing sides in, or polarizing, the problem.

So 001 514

KATHLEEN ARCHIBALD: COMMENTS ON THOMAS MILBURN'S PRESENTATION, "PROBLEMS OF INTEGRATING ACADEMIC DISCIPLINES IN THE STUDY OF WAR, VIOLENCE AND SOCIAL CHANGE"*

I think the study of conflict has developed as an interdisciplinary area, at least relative to other issues. But if you ask how do we take it to the next steps, I think it is an institutional problem. You have to face the fact that it is very hard to do interdisciplinary work in the University, even in the so-called interdisciplinary institutes. This is very noticeable when one moves, as I have done, from a university to a place like RAND: RAND doesn't do its interdisciplinary work well, but it is certainly way ahead of most Universities. I think it is easy to say what we need to do; it's much harder to start the change process that allows one to make institutional arrangements for the interdisciplinary work.

It seems to me that beyond doing good interdisciplinary research, there is another important step: that is moving work in areas like conflict and change into a more useful applied direction. I'm partly saying that to have a good applied discipline it has to be interdisciplinary. And I think in the field of conflict we have gone some distance toward solving that. What we haven't come very far in solving, however, is how do you do good applied work in the field.

There are alternatives for the researcher or expert working in the field. One alternative is the one generally followed by social scientists in universities who are concerned about war/peace issues. They want to work in the area; but essentially they stay oriented to their discipline, hoping that what they do can be made relevant by a middleman who will "translate" their findings so they will be useful for the decision-maker. The research, therefore, is not designed to meet the needs of the practitioner, or decision-maker, because the researcher isn't sure himself how that translation should be done. I call this approach "discipline oriented" for although the academic may wish to make a practical contribution as well as a disciplinary one, in fact, the thing that gives primary shape to the research he does is his discipline and not the demands of the practical problem.

A second kind of orientation takes the boundaries of the problem being looked at as the boundaries of the client. I call this "client oriented," and it is represented by the human relations approach, the organizational development approach, the client change approach. In this orientation the consultant attempts to make an organization better

*Kathleen Archibald, The Rand Corporation. These comments were made at the Inquiry, "The Utilization of Scholarship in Teaching about War, Peace and Social Change," March 1970, San Francisco. Sponsored by the Center for War/Peace Studies, in cooperation with the American Orthopsychiatric Association, the International Studies Association and the Diablo Valley Education Project.

able to cope with its problems. So that, for example, with this orientation if you're working for OEO, you don't worry about the task in the real world that OEO is struggling with. You are trying, instead, to make OEO good enough so that it can solve its own problems. In other words, you don't try to solve its external problems; you try to solve its internal problems. The underlying model is borrowed from psychoanalysis, only now applied to an organization.

A third kind of model is what I call "decision oriented." This seems to me closer to the RAND way of operating, to the way operations researchers and economists tend to consult. They are looking at the real problem in the real world. But they don't look at the total problem and all that leads into it because they are, in fact, working for a decision-maker, let's say OEO. I call this approach "decision oriented" as distinct from "problem oriented," because the work is undertaken at the intersect of (1) the real problem in the real world and (2) the capability of the client to deal with the problem. With this orientation you don't tackle the whole problem but only those aspects of it which are under your clients jurisdiction. So when you are working with OEO on reading improvement, the problem is defined in terms of the variables that OEO controls or can affect. The difficulty here is that, while your research may have some influence in improving the situation, its utility is limited by the capabilities of your client. If other decision-makers control most of the important levers that could ameliorate the problem, your research will have little impact.

I think the best we can do at the moment is to combine the decision oriented approach with the client oriented approach. The decision oriented approach is pretty good at turning out a nice blueprint for the client specifying the alternative he should prefer. But what is not considered is whether the client's organization, at the operational level, is capable of doing the job. So what comes out of the operational end of the hopper often looks very different from the original blueprint. The client oriented approach has a lot to offer on this kind of problem, so I expect a lot of progress could be made by combining the decision oriented and client oriented approaches. This is assuming, of course, that we can solve the interdisciplinary problems. Part of the hang-up here is disciplinary; it's the psychologists and social psychologists who tend to move to the client oriented approach and the economists who almost automatically move the other way. The resulting communications problems are partly disciplinary.

I'd like to end by suggesting a further refinement; can't we think about an orientation that is really "problem oriented?" In this approach, the problem would be analyzed first and the key levers of action identified, then the researchers would approach and work with those who control these levers; that is, those decision-makers who have what it takes (authority, resources, influence, etc.) to make significant progress towards solving the problem. I can think of few cases where any thing close to this has been done. And, of course, one of the reasons is--who supports the researchers while they do it? It's not the kind of work that, currently, can be done within a university. It's expensive and you'd have to spend a lot of time looking at levers of action with a very

applied orientation. So what you need to do this kind of work is an applied research institute that is not dependent on client contracts for its support.

Perhaps we can think about ways to accomplish this. Ways to move in on a problem and say, "Okay, who are the clients we work with on this?" "What are the levers of action we move on that?" This means thinking of large institutional questions - because, unfortunately, you can't get ten good-hearted bright people together for a few weeks or months and solve the problems. You have to have some sort of an organization that permits good people to work on important practical problems on a long term basis.